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MODERN EVIDENCES FOR DIFFERENTIAL MOVEMENT OF CERTAIN POINTS ON THE EARTH'S SURFACE[1]

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THE subject of our discussion is one of those problems that not only straddles the border line between departments of knowledge, but transgresses far into the adjacent territories of several sciences. I am reminded of an apt warning for which Bulwer-Lytton is responsible, not to fall "into the error of the would-be scholar—namely, quoting second-hand." Making no pose as a geologist, however, I must confess that what I may have to say in regard to physiographic examples of differential displacements in the earth's crust will have to be from such little knowledge as I have been able to borrow from you geologists, and I venture to proffer such second-hand information only on the ground that we have a board of experts among us to whom questionable points may be referred subsequently. My excuse for venturing upon this subject comes from a very vital interest that must concern every student of the earth.

Certain investigations with which I have been more or less engaged for the last fifteen years seem to lend promise that the problem of lateral shifts in the earth's crust may be helped toward solution by the continued observation of the latitude and longitude of certain presumably fixed points on the earth's surface. In that unique organization of students of the earth sciences, the American Geophysical Union, experts from many different, but interrelated fields commingle freely. It would appear that it was through such commingling some astronomers became contaminated by some geologists perhaps with a fond hope that a little contamination of astronomy with geology might be of mutual benefit. In the time at our disposal I hope I

[1] An address presented to the Geological Section of the New York Academy of Sciences on April 3, 1944.

may be able to treat a very interesting geophysical problem with sufficient decorum so as neither to offend, on the one hand, any astronomers present who may wish their observatories to stay put, nor on the other hand unduly encourage those geologists who would like to have whole continents moved.

The most dramatic evidences for translational displacements in the earth's crust are still to be found in the geologists' picture album, and I am indebted to our colleague, Professor Thom, for the loan of certain photographs that portray graphically displacements of many feet along fault lines familiar to every geologist. Not all these displacements are sudden occurrences such as accompany great seismic disturbances. The slow crawling of the crust in the neighborhood of the San Andreas fault is well known to the petroleum industries whose pipe lines have bent and buckled and often broken under the stresses and strains of the earth's crust. I have inquired of my geological friends for evidence in other parts of the country of equally striking displacements—so far without very encouraging results. It would appear that California, in keeping with her other superlatives, may continue to boast, as the state par excellence of Hollywood Stars, gigantic telescopes and of maximum terrestrial displacements.

Of course, looking back into geologic history, we have a panorama presented of a long series of depressions and upheavals of overthrusts and underthrusts which have caused the characteristic formations of the orogenic zones. Dr. Bucher has stressed the role of thrust-sheets in orogenic deformation and would cite as a specific example the Cumberland thrust-block lying across the tri-state corner of Kentucky, Virginia and Tennessee.[2] Here a slice of rock over 120 miles long, some 20 miles wide and a mile in thickness has been sheared off, closely parallel with the bedding and pushed along a distance of at least 7 miles. In addition, the whole slice has apparently been rotated, producing cross wrinkles.

The presence of great rift or shift zones in continental shield-areas was graphically depicted by Dr. A. W. Jollife at a recent symposium.[3] Quoting Dr. Jollife,

> Many faults in the Canadian Shield are known to follow essentially rectilinear vertical fractures that extend for distances up to several hundreds of miles. None of them is known to be marked by major vertical displacements whereas a number show strike-slip movements amounting to several miles. From this it is inferred that the fault movements were dominantly horizontal of the same general nature as those along the San Andreas Rift. Movements along many of the Shield faults were recurrent, the most recent being late Pre-Cambrian or even younger in age.

While geologists generally recognize that large differential horizontal displacements have taken place in the past, probably many still believe that such movements between crustal segments are negligible at the present time. Professor Thom,[4] in a recent report of his special committee on the Geophysical and Geological Study of Continents, stresses that "Evidence has now accumulated to a point where it is reasonably clear that an appreciable differential crustal shift is still going on in different parts of the world. And it therefore seems highly expedient that adequate data as to nature, magnitude and disposition of these relative movements should be obtained forthwith because of the critical bearing of these data upon over-all geodetic and cartographic programs and operations."

That a certain recurrence at more or less definite intervals, or a cyclical rhythm, in large crustal displacements has taken place in geologic time seems to be beyond contention. Accumulating evidence continues to point out that such movements must still be taking place, but when large masses of terrain are involved, especially in regions unscarred by fault lines there is a difficult problem in determining either the direction or the amount of such movements as may be taking place currently. It is for this reason that the geologists look to the astronomers to see what evidence there is, if any, that displacements have taken place in very recent times. To interrelate more closely the determination of the crustal movements with the astronomical method of finding changes in position, we perhaps should remove ourselves sufficiently far from the earth itself to get the cosmic picture of the planet with which we are concerned and its immediate environment.

To use an analogy, those of us who inhabit the earth find ourselves aboard a ship on the high seas of space. Contrary to the opinions of the earlier astronomers, this spherical ship is far from fixed. At this very moment we are being carried eastwards with the earth's daily rotation at a velocity in this latitude of about 600 miles an hour. Meanwhile, the good Ship Earth is steaming ahead in its orbit about the sun with a velocity of 20 miles a second. The sun, however, which is the flagship of our fleet of planets, is steaming ahead on its course among the nearer stars at a velocity of some 12 miles a second, or 40,000 miles an hour. The sun in company with other suns comprising a still larger squadron is in turn sailing through space at a speed of several hundred miles a second towards the lights of other celestial ships seen at the

[2] W. H. Bucher, "Deformation of the Earth's Crust," p. 244. Princeton University Press. 1933. *Trans. Amer. Geophys. Union*, p. 697. 1934.

[3] A. W. Jollife, *Trans. Am. Geophys. Union*, pp. 699–707, 1942.

[4] W. T. Thom, Jr., *Trans. Am. Geophys. Union*, p. 304, 1943.

limit of the horizon of our greatest telescopes. Meanwhile as passengers aboard the earth, all quite unconsciously we are being tossed to and fro due to certain peculiar wobblings of the earth's axis, and now even the deck of the ship on which we stand—the crust of the earth—proves itself unstable as it trembles now and then due to internal catastrophes within the ship's hold.

As passengers aboard our frail celestial craft we are subject to all the cosmic forces known and unknown that play about in the mysterious ocean of space. Only as we examine our environment can we hope to come to anything like a complete knowledge of some of the disturbing forces that are operating upon the earth. It is to astronomers, as the navigators upon the bridge of this strange ship, that our fellow passengers look to tell the ship's position, define her course, and speed. Only by observations of those lights exterior to the earth—the distant stars—can we hope to evaluate not only the motions of the earth, but movements that may be taking place in the earth's crust. While in the last analysis, we can never hope to define the absolute motion of ourselves among a vast system of moving stars, we can take courage at the degree of precision which may be attained from astronomical observations. For those not intimately acquainted with the applications of practical astronomy, it comes perhaps as something of a surprise when we consider that it is possible from looking at the stars to determine one's position on the surface of the earth with an uncertainty of but a very few feet. More specifically, from a series of careful observations we can determine where we are north and south—latitude—within one foot or two; and in the east and west direction—longitude—within an uncertainty of perhaps four or five times that amount.

The problem of the determination of latitude is fundamentally very simple since an observer is as far north of the earth's equator as his zenith appears from the celestial equator, the zenith being determined by the direction of the plumb line at a given point. One needs only to attach a telescope in a sufficiently elegant manner to a level bubble, and the zenith can be located among the stars whose positions have been catalogued with respect to the celestial equator. Unfortunately, however, the positions of the stars can be determined only if the latitude of the observatory is known so that it is only through a series of successive approximations of a large number of cooperating observatories that the positions of the stars can be known so that the latitude of any station can be absolutely determined.

VARIATIONS IN LATITUDE

For finding small variations in latitude, at a single station the true value of the star positions is not necessary, provided the star has the courtesy to remain stationary during any series of latitude observations in which we are interested. From a long series of observations made at a selected number of stations operated through international cooperation along the 39th parallel it has been possible to evaluate with great accuracy small variations in latitude. It has been determined that the pole of rotation of the earth is constantly shifting within the earth itself. This wandering of the pole first announced by Chandler, and later by Kustner of Berlin, is of a somewhat cyclical nature, having a period of approximately 428 days with a superimposed annual component. The migration of the earth's pole of rotation about the pole of its figure has never yet been observed to exceed a radius of 30 feet. An inter-comparison, however, from time to time of the latitudes of the several stations involved, namely, Gaithersburg, Carloforte, Chardjui and Mizusawa, have revealed certain discrepancies that may well have indicated crustal displacements. In the reduction of the last International Latitude Survey, Kimura[5] bluntly stated that with the position of the pole determined from all the other latitude stations excluding Mizusawa, the latter station could be considered to have moved southwards by about three meters from the position occupied ten years earlier. On the other hand, certain geodesists are loath to accept such as evidence, preferring rather the alternative interpretation that the discrepancy lies in errors in the adopted positions of the stars used in the reductions of the observations. With a certain daring impishness, however, I venture to remark in paraphrase, "The trouble, dear Brutus, may not be with our stars, but with ourselves, that we are moving."

It should be emphasized that in determining latitude we are concerned with the precise location of the zenith among the stars. Anything that influences the direction of the plumb line, peculiar to the observatory station, will show up as a variation in latitude. A shore station, subject to the loading of the ocean tides, will have its plumb line periodically deflected in the course of a lunar day, by the gravitational attraction of the rising tide of water off shore. The tilting of the land which results from the hydrostatic pressure will be readily manifested on a tilt-meter, but this tilt will not displace the direction of the plumb line or cause a variation in latitude, unless there is a lateral shift along the equipotential surface. The distortion of the plumb line due to the attraction of a mass of ocean water off shore, has been worked out and can not much exceed one one-hundredth of a second of arc. A tilt of the coast line, due to this loading, may amount to a considerable fraction of a second of arc for some distance back of the shore line itself.

[5] H. Kimura, *Proc. Imperial Acad. Tokyo*, 10 No. 10, 1935.

The moon, of course, causes a calculable tide in the solid earth as well as in the oceans. The celebrated Michelson-Gale experiments carried out on the grounds of the Yerkes Observatory in 1913 to 1915 made possible the observation of well-defined earth tides through a tilt-meter consisting of a horizontal pipe 500 feet long half filled with water. The microscopic tides caused in this pipe were measured by an elaborate optical method, and revealed amplitudes in the waves less than those calculable on the assumption of an unyielding earth. The discrepancies between the observed and the calculated values of the tides made possible the evaluation of the amount of yield in the solid earth. From the results it was found that the rigidity of the earth as a whole was perhaps a little greater than that of steel.

It should be pointed out in this connection that such a calculation must of necessity be made for the earth as a whole as a homogeneous body. Were there a different yield in the crust than in the earth's interior there would be no way of determining it by this method. Again it should be remembered that the observations revealed tilts only. Were there an actual rhythmic translation of the crust taking place as the tidal wave passed the instrument, this could not have been detected by the water pipe. On the other hand, any horizontal displacement or movement of the superficial crust along the equipotential surface under the influence of the tidal force would cause the plumb line to shift its position and a variation of latitude would be detected by the zenith telescope.

It was in the interest of trying to discover whether or not such tangential displacements as might take place could be detected by astronomical observation that I spent some years in investigating latitude observations for any variation that might appear to correlate with the moon's position. The results of the investigations indicated what appeared to be a definite lunar correlation in the Gaithersburg series made with a photographic Zenith tube in 1913–1914.[6] Further studies revealed similar correlations for the observation made at Ukiah. The amplitude of the variations found were of the order of several hundredths of a second of arc, corresponding to lateral shifts from two to five feet in the North and South direction.

These results were considerably larger than those that would be anticipated on the assumption of an earth tide calculated theoretically for the earth as a whole. Others who have made a rigid harmonic analysis of latitude observations for a lunar tide found results consistent with a theoretically calculated earth tide. It should be remarked, however, that in my study the form of the curve of latitude with the altitude, or time of lunar day, has by no means repeated itself completely in successive years. Patterns differ both in phase angle and in amplitude. Such results would not be inconsistent with tidal forces encountering resistance to deformation. Such an effect could be completely masked when several years of observation are subjected to a rigid analysis that presupposes symmetric harmonics.

It is of particular geophysical interest to note that the stations conveniently selected by the International Union for determining latitudes happened with one or two exceptions to lie in orogenic zones. The stations originally selected were Gaithersburg, Maryland; Cincinnati, Ohio; Ukiah, California; Mizusawa, Japan; Chardjui in the Himalayas; and Carloforte off Sardinia.

Observations at the Cincinnati observatory were early abandoned because of discrepancies, and large probable errors in the results. The Gaithersburg observatory was next abandoned for a time, but after a lapse of several years has again fortunately been reoccupied. The location of the other stations are, without exception, in extraordinarily interesting sections geologically, and perhaps could not have been better placed if the astronomers had been hunting for discrepancies that could be caused locally through crustal displacements. The astronomers, on the other hand, are naturally reluctant to attribute latitude discrepancies among the several stations in question to differential displacements. The object of the latitude project was patently to observe the motion of the earth's pole of rotation, rather than to try to discover crustal movements in the neighborhood of the telescope, which they presumably believed fixed on masonry piers where they were intended to stay put.

One further comment on this problem of latitude before discussing the east-and-west coordinate of longitude. Some twenty years ago Dr. Lambert, of the Coast Survey, presented an exhaustive discussion of the latitude observations at Ukiah, California,[7] in which it was shown that from 1900 to 1915 the latitude of Ukiah had more or less steadily increased some 0″.15 during the decade and a half. For some reason it seems easier for astronomers to allow the imaginary earth's pole to slip within the crust than to assume that the actual territory surrounding Ukiah had, during this interval, inconsiderately slipped north 15 feet; so the interpretation of the result at first was that the North Pole of the earth had been slowly migrating affectionately toward North America. This did not appear plausible, so it became more or less the consensus of opinion that the trouble after all was with the stars, that they were drifting. By arbitrarily cor-

[6] H. T. Stetson, *Nature*, 123: 127, 1929; SCIENCE, 69: 17, 1929.

[7] W. D. Lambert, *U. S. Coast and Geodetic Survey* Special publication 80, 1922.

recting the declination of the stars used for observation, one could make the earth's North Pole behave more seemly, and thus not unduly excite the Californians over an unwarranted skid toward the North Pole. That, at least, placed the chief worry in the hands of the astronomers, and relieved the geologists and real estate agents of any cause for alarm. Who, other than the astronomers, care especially whether the stars are drifting or not? We can keep that secret within the family. However, more seriously, even some astronomers in off moments have geological interests, and it is coming to be recognized that while it is difficult to prove, or disprove, some translational movements in the region of the latitude stations, some of us are advisedly considering other methods of observation that do not presuppose a knowledge of the absolute declination of stars, that in turn must of necessity be found only from assuming values for the latitudes of the observatories from which the observations are made! The only apparent method of accomplishing this is the observation of stars above and below the pole. This can be well done in high latitudes. Unfortunately, on account of atmospheric refraction, one can not expect the degree of precision in such a method as is attainable by the adopted Talcott method of zenith observations.

The possibility of comparing latitudes of certain well-established stations after an interval of 70 or 80 years interested Dr. Frank Schlesinger, late director of the Yale Observatory, and I quote from a letter written by him to the director of the Coast and Geodetic Survey on May 5, 1930:

> As you are doubtless aware, the problem of variations of latitude has recently taken on a new aspect of interest, owing to the possibility of a secular change, as well as periodic changes, in the position of the pole. This is one reason why the resumption of observations at Gaithersburg has become so important. An independent way in which considerable light can be thrown upon this question occurred to me several years ago, and I have discussed it with a number of my colleagues, including Doctor Bowie, chief of your division of geodesy. The plan is to reoccupy for one or two nights each, a number of the stations at which observations for latitude were made 20 or more years ago by the Talcott method. The same stars should be observed now as were used then. From the results obtained in this way we may reasonably expect light to be thrown upon the following questions: (1) The actuality of a secular motion of the pole; (2) the possibility of slow continental, or at least general drift; (3) a better determination of the systematic errors in our adopted systems of proper motions of the fixed stars.

As a result of this letter three stations in the United States whose latitudes had been well defined were selected for reoccupation—Mt. Pleasant in Maine, Mt. Tom in Massachusetts and a station in Des Moines, Iowa. The latitude of these stations had been accurately determined in 1851, 1862 and 1869 respectively. In all cases the Talcott method of the zenith telescope had been used. The results of this reoccupation of these stations showed that, using the same stars and the best available star positions from the well-known Boss catalogue, Mt. Tom in Massachusetts had decreased in latitude by 0″.99; Mt. Pleasant in Maine showed an increase in latitude by 0″.29; Des Moines, Iowa, showed a decrease in latitude by 1″.49.[8]

Translated into linear distances the evidence was that in the last three quarters of a century Mt. Tom had slipped about 100 feet south, while Mt. Pleasant in Maine had gone north about 30 feet, and Des Moines, Iowa, indicated a southernly drift of 150 feet with respect to the earlier position.

One of the few major observatories now remaining that has continued positional astronomy is the Lick Observatory, Mt. Hamilton, California. An analysis of the latitude of this observatory based on meridian circle observations from 1893 to 1918 was made some years ago by Professor Lawson and published in the bulletin of the Department of Geology of the University of California.[9] Professor Lawson's discussion exhibited evidence of an increase in latitude of 0″.40 from 1893 to 1903. In 1903 a conspicuous drop of 0″.63 in the value of the latitude occurred; an interval coincident with the earthquake of August 2, 1903. The somewhat more scattered observations from 1903 to 1918 showed a subsequent increase in latitude so that by 1918 the position of the observatory was essentially that at the beginning of the series. Dr. Campbell, then director of the Lick Observatory, while considering that small changes in latitude may have taken place at about the time of the earthquake, believed that a certain amount of the change pointed out by Professor Lawson could be accounted for by possible accidental errors. The otherwise apparent steady drift of the Lick Observatory northwards is quite consistent with, though somewhat larger than, the increase in the latitude of Ukiah, and I might also add Gaithersburg, which Dr. Lambert of the Coast Survey has discussed at length. During the earthquake of 1906 when dislocations of six meters were reported no definite proof of a change in the position of Mt. Hamilton was found from Lick Observatory observations. It is surprising that from triangulations made before and after 1903 the report was made that no certain evidence appeared for a shift in the position of the Lick Observatory.

VARIATION IN LONGITUDE

Fortunately, when we come to the consideration of detecting movements of the earth's crust in an east-

[8] F. W. Darling, *U. S. Coast and Geodetic Survey Special Publication 173.*

[9] Lawson, *Univ. of Cal. Pub. Bull. of Dept. of Geology,* 12: No. 7, 1921.

and-west direction by astronomical observations, we do not have to be concerned with the absolute positions of the stars. The difference in longitude between two places is nothing but the equivalent of the amount of time that elapses between the moment when a star is seen on the meridian of an observer at one station and the moment when it is seen on the meridian at a second station, west of the first. If an observer at the Naval Observatory in Washington learns by wireless the exact instant when a star has been observed as it crosses the Meridian of Greenwich, and finds that 5 hours 8 minutes and 15.78 seconds have elapsed before it crosses the Meridian of Washington he knows that the longitude west of Greenwich is 5 hours 8 minutes 15.78 seconds, or an equivalent in arc of 77° 0′ 3″.7. If from time to time this interval should vary in the last decimal place, he might suspect, if he is mobile minded, that the longitude between Greenwich and Washington varied.

While in the problem of differences of longitude we have delightfully dismissed the problem of accurate knowledge of star positions, we have introduced yet another pernicious uncertainty. This uncertainty is the degree of accuracy with which we can call up London and find the exact moment of the Meridian passage of any given star. Of course, in practice many stars are observed on both sides of the Atlantic, and the best possible values for the corrections for the Greenwich Observatory clocks are made that such observations allow. The exact time is then broadcast at stated intervals by radio which can be picked up by our Washington observatory and compared with the Washington time of transit of the same stars. Meanwhile, however, we have had to rely upon some mechanism that would keep accurate time during the five-hour interval from the moment when the stars are observed in Greenwich, and the time when the same stars can be observed in Washington. Fortunately, we now have rather good timepieces which can be trusted with an astonishing degree of reliability. In the old days of longitude observations time signals had to be passed through the Atlantic cable and large uncertainties in the reception of time signals from England were inherent in the manner of the electric performance of the cable. Time signals can now be exchanged by radio waves which, if one does not know too much about them, may be assumed to travel with the velocity of light. Allowing for all knowable uncertainties, one may state without exaggeration that time can be determined on two sides of the Atlantic and signals exchanged with a probable error of only about ± 0.007 seconds.

A few years ago Mr. Alfred Loomis and myself became interested in what appeared to be unaccountable variations in time observed on the two sides of the Atlantic; variations many times larger than any errors of observation or transmission by radio would seem to allow. Such variations furthermore showed a more or less rhythmic oscillation over periods of days and weeks. At that time we felt that there was fairly good evidence that the position of the moon was definitely related to small shifts in latitude. We therefore undertook to see whether or not the small differences in determination of time on the two sides of the Atlantic, that might represent small variations in longitude, could also be attributable to the moon. The presentation of the first paper on this subject to the American Astronomical Society[10] at a joint meeting with the American Association for the Advancement of Science at Atlantic City in 1932 so startled *The New York Times* that their front page the next morning startled the authors, the astronomers and the rest of the world with the idea that the two English-speaking peoples on the two sides of the Atlantic were sometimes nearer to each other by 60 feet, and sometimes further away from each other by 60 feet, dependent upon what the moon had to say about it. Additional investigations by Mr. Loomis and myself, and later published by the Royal Astronomical Society[11] seemed to collaborate our earlier findings. The variation in time which appeared to correlate with the hour angle of the moon, or the hour of the lunar day, represented recurrent discrepancies of about five hundredths of a second of time. If interpreted as a lineal change of distance in the east and west direction this would correspond to a range of 64 feet. While we were cautious in not forcing any geological interpretation as an explanation for the phenomenon, we could not but speculate as to what hypothesis could be sanely found to explain a to-and-fro movement of the earth's crust, were such really to exist. Much water has gone over the dam since this was first published. As to this hypothesis there have been, geologically speaking, periods of submergence and emergence, of inundation and denudation. I think I can very briefly summarize what has happened since.

I regret that it is not possible here, on account of time, to present more details of the evidence which Mr. Loomis and I presented for a correlation between the position of the moon and the suspected variation of longitude. I should emphasize that we called attention to the fact that our table showed a marked seasonal variation about which it was remarked we should have more to say later in another paper. We did, however, publish date by date the reduction of the

[10] A. L. Loomis and H. T. Stetson, *Publications American Astronomical Society*, 7: 177, 1932.

[11] A. L. Loomis and H. T. Stetson, *Monthly Notices Royal Astronomical Society*, 93: 444, 1933; "Further Investigations of an Apparent Lunar Effect in Time Determinations," 95: 454, 1935, MN-RAS.

time signal observations upon which our findings were based, in the hope that others challenged by the problem presented might be interested to make further contributions.

We did not have long to wait. A Japanese investigator Kawasaki,[12] utilizing our tabulated data, soon took the floor and showed that the reversal in phase of our curve of longitude variations which we had attributed to the change in the declination of the moon from the north to the south side of the equator could be accounted for by an annual term of unknown origin, the annual term being the seasonal variation in the values of longitude variation to which we had called attention.

We now know that Kawasaki was wholly right in showing that under the conditions peculiar to the times when the star observations were made such an annual term could produce the effect observed so far as the reversal of phase in the resulting curves was concerned. He made no attempt, however, to account for the occurrence of the annual term, or the seasonal variation, or to deny that such an effect could be related to the moon.

(*To be concluded*)

OBITUARY

WILLIAM TITUS HORNE

WILLIAM TITUS HORNE, professor of plant pathology in the University of California, died on April 12, 1944, after an illness of a few days. He was born near Kankakee, Ill., on November 8, 1876. Professor Horne received his early education in the public schools of Bennett and Lincoln, Nebr., and then attended the University of Nebraska from which he obtained a bachelor of science degree in 1898. After serving as instructor in the Nebraska Wesleyan University and University of Nebraska Farm School, he was employed in a fish hatchery at Karluk, Alaska, in 1901–1902. He took graduate study at Columbia University as fellow in botany in 1903–1904. At the Cuban Agricultural Experiment Station from 1904 to 1909 he served as assistant and then chief of the department of plant pathology. He came to the University of California at Berkeley as assistant professor of plant pathology in 1909 and was acting head of the division of plant pathology in 1919–1920. He transferred his activities to the Citrus Experiment Station in 1928, where he became associate professor and then professor of plant pathology in 1939. Here he had a long and useful service, especially in the field of avocado and subtropical diseases.

While at Berkeley, many students felt his kindly influence and careful instruction, and numbers of them are now active and prominent in scientific research, teaching and commercial life.

At Riverside he has made notable scientific contributions, especially to the better understanding of avocado disease problems. He made a host of friends by his friendly attitude and gentle, kindly life, not only among his immediate associates but among the growers as well. An excellent example of the appreciation of his work for the avocado industry is a quotation from a scroll presented to him on May 3, 1935:

> The Avocado Department of the Los Angeles County Farm Bureau takes this means of expressing to William Titus Horne, Associate Professor of Plant Pathology at the University of California, its deep appreciation of the years of untiring and unselfish work devoted by him to the problems of the avocado industry. Much of this work has been beyond the requirements of his position. His modest, unassuming manner and deep human interest in the problems of the growers has endeared him to all of us.

Later, in 1938, he was asked by the California Avocado Association to present the medals at Atlixco, Mexico, in recognition of the sending of the Fuerte variety to California. In the same year his colleagues in plant pathology made him president of the Pacific Division of the American Pathological Society. He was a member of the American Association for the Advancement of Science, the American Phytopathological Society, the Mycological Society of America, the California Botanical Society, the Torrey Botanical Club, Sigma Xi and Alpha Zeta.

One of his most important publications since coming to Riverside was his 1934 bulletin on Avocado Diseases. He had ready at the time of his death a completed manuscript on "The Diseases of the Guava," which is being edited for publication by the University of California.

In 1906 he married Mary Tracy Earle, sister of the late Professor F. S. Earle, at Santiago de las Vegas, Cuba. Their beautiful home and garden at Riverside, from which friends received innumerable gifts of flowers and fruits, was an expression of their kindly life and endearing hospitality.

H. S. FAWCETT
L. J. KLOTZ
P. A. MILLER

AGRICULTURAL EXPERIMENT STATION,
RIVERSIDE, CALIF.

RECENT DEATHS

DR. DAVID EUGENE SMITH, professor emeritus of mathematics at Teachers College, Columbia University, died on July 29 at the age of eighty-four years.

DR. JOSEPH CHRISTIE WHITNEY FRAZER, research

[12] S. Kawasaki, *Monthly Notices Royal Astron. Soc.*, 96: 818, 1936.

professor of chemistry at the Johns Hopkins University, died on July 28 at the age of sixty-eight years.

DR. WILBERT B. HINSDALE, anthropologist and professor emeritus of internal medicine at the University of Michigan, dean of the Homeopathic Medical College, died on July 26. He was ninety-three years old.

DR. FRANK J. TONE, chemical engineer, chairman of the Board of Directors of the Carborundum Company, died on July 26 at the age of seventy-five years.

SIR RALPH FOWLER, since 1932 Plummer professor of mathematics at the University of Cambridge, died on July 28 at the age of fifty-five years.

SCIENTIFIC EVENTS

THE UNIVERSAL DECIMAL CLASSIFICATION FOR LIBRARIES

THE American Standards Association announces that it has on hand copies of the first four completed volumes in the Universal Decimal Classification for libraries, being published by the British Standards Institution as a British Standard. This work, which may extend to additional volumes, sets up a system for classifying library books that can be applied to special subject libraries, as well as to general libraries all over the world. Since the use of numbers has eliminated language as a barrier, any librarian should be able to make use of a library of another country where this system is used and feel perfectly at home.

The Universal Decimal Classification is actually a highly detailed enlargement of the Dewey Decimal System, developed along somewhat the same lines. The universal classification is a great deal more specialized and more intricate. It will prove useful, therefore, primarily to experts and libraries in specialized fields, since it is designed to accommodate an indefinite number of subdivisions.

This classification was prepared by the Federation International de Documentation with the help of leading experts in many fields. It has been adopted by the International Federation of the National Standardizing Associations for the classification of its documents, and is the most widely used of any single classification. Both the British Society for International Bibliography and the Association of Special Libraries and Information Bureau requested this English edition of the Universal Decimal Classification, with the approval of the Education Foundation, Lake Placid Club, New York, publishers of the Dewey Decimal Classification. French and German editions have already been published.

The subdivision of the "subject" classes follows the accepted principle, "from groups of wide extent to groups of less extent," that is, there are main classes and auxiliary classes. The main classes, like philosophy, religion, social sciences and art, are broken up into auxiliary classes such as psychology, Christian churches and sects, law, architecture, and these classes may be further subdivided.

From time to time various readjustments are expected to be made. The Universal Decimal Classification is under the continual supervision of experts in an effort to keep it up to date. The British Society for International Bibliography and a series of international subject secretariats are two of these groups, although the Commission and Secretariat of the Classification of the Hague has the final decision for amendments and extensions.

While it is necessary that fully developed tables be placed at the disposal of experts and specialists, an abridged system formed by the major groups with simpler symbolic representation can be used for the classification of books in smaller libraries.

The classification is especially valuable in specialized files owing to its complete detail. It is also well adapted for use in connection with various automatic card sorting machines.

EDUCATION AND RESEARCH IN AVIATION AT THE UNIVERSITY OF ILLINOIS

THE first meeting of the University of Illinois Advisory Board on Aeronautics, which is composed of members distinguished for their work with various aspects of aviation and related activities, was held at the University Club, Chicago, on June 30.

The board was appointed to assist the university in the organization and development of a comprehensive program of education and research in aviation. It includes not only technological phases, but also economic, sociological and medical aspects.

Much of the activity of the university will center around its new airport now under construction. There will be three runways, each 5,300 feet in length and 150 feet wide. Space is available for extending the runways to 8,300 feet, as well as for additional runways. The facilities planned are sufficient to accommodate any aircraft now in use, including the B-29. The university has purchased 763 acres of land up to the present time and the area will be expanded as needs develop.

The last session of the Illinois State Legislature appropriated $250,000 for land and $500,000 for the construction of buildings. In addition, the Civil Aeronautics Administration approved contracts amounting to $1,354,000 for improvements, including runways, drainage, taxiways and other construction.

The airport is the first unit of the planned facilities for education and research in the many phases of aviation. The ultimate aim is to have in one university the plant and personnel to cover the economic, sociological and medical problems, as well as the technological problems of aeronautics. The new airport will be open for military and commercial use, for civilian flying and will at the same time serve as a research laboratory for the university.

The official announcement of the plans gives the following objectives for the university in relation to aviation:

1. To serve the industry by providing education for such college-trained personnel as are needed in the design, construction and production of all types of airplanes and engines; in the whole field of commercial air transport for both foreign and domestic service; and in airport design, construction, operation and maintenance.

2. To develop research personnel for private, commercial, government and university laboratories.

3. To provide teacher-training programs for high-school teachers, both specialists who teach aviation courses and non-specialists who teach high-school courses related to aviation.

4. To develop and direct off-campus educational programs for technical or industrial personnel and teachers, and for adults interested in the social, economic and political implications of aviation.

5. To provide short courses on the campus to educate university graduates in the newest developments and needs of aviation, and to keep them abreast of changing needs and knowledge.

6. To provide flight instruction for limited numbers of properly qualified students under sponsorship of the CAA or the Reserve Officers' Training Corps. (For example, high-school teachers who expect to handle special aviation courses should acquire some slight experience.)

7. To develop, in cooperation with the aviation industry, the CAA, the Army and Navy air forces and with other interested national agencies, cooperative research programs related to air transportation.

THE COMMITTEE ON PHARMACEUTICAL RESEARCH OF THE AMERICAN PHARMACEUTICAL ASSOCIATION

THE Committee on Pharmaceutical Research of the American Pharmaceutical Association announces the availability of certain limited funds for research grants.

These grants are to be made by the council on the recommendations of the Committee on Pharmaceutical Research. They are to be made on the premises:

(a) the extent which the award will serve to promote pharmaceutical research;

(b) whether the award supplements the American Pharmaceutical Association laboratory program;

(c) the qualifications of those who will perform the work for which the award is made and the facilities of the laboratory where the research will be conducted;

(d) preference will be given to applications wherein the award will supplement a contribution from the institution or laboratory in which the research will be conducted.

Those interested in being considered for the above awards should communicate promptly with the chairman of the Committee on Pharmaceutical Research (Ivor Griffith, 43d and Kingsessing Ave., Philadelphia 4, Penna.) who will send an application blank which, when returned, will supply all the details that will enable the committee to evaluate carefully the projects listed in the application. These applications should be forwarded promptly so that they can be given early consideration.

IVOR GRIFFITH,
Chairman of the Committee on Pharmaceutical Research

SYMPOSIA OF THE AMERICAN CHEMICAL SOCIETY

A SYMPOSIUM on "Frontiers in Biochemistry" will be held by the Division of Biological Chemistry at the one hundred and eighth meeting of the American Chemical Society, which meets in New York City from September 11 to 15. It is announced that twenty-nine papers will be presented at five sessions. The subjects to be discussed include human plasma, penicillin and vitamins. Dr. Elmer M. Nelson, of the U. S. Food and Drug Administration, Washington, D. C., chairman of the division, will preside. Institutions represented on the program are Columbia, Cornell, Harvard, Minnesota, Missouri, Pittsburgh, Rutgers, Stanford, Texas, Wisconsin and Yale, the Rockefeller Institute, New York; the Mayo Clinic, Rochester, Minn.; Lankenau Hospital, Philadelphia; the Massachusetts General Hospital, Boston; the U. S. Department of Agriculture and Distillation Products, Inc., Rochester, N. Y.

Among the speakers and their subjects are the following:

Dr. Henry C. Sherman, of Columbia University, now working with the U. S. Department of Agriculture, will present a paper on "Nutritional Principles in Wartime Food Problems." "Studies on Vitamins B–10 and B–11" will be reported by a group from the University of Wisconsin composed of Drs. C. A. Elvehjem, J. M. Briggs, Jr., T. D. Luckey and E. B. Hart. "The Chemistry of Muscle During Shock" will be the subject of a paper by Jesse L. Bollman and Eunice V. Flock, of the Mayo Foundation.

Dr. Karl Meyer and Eleanor Chaffee, of Columbia University, will speak on "Biologically Active Derivatives of Penicillin."

Dr. Rollin D. Hotchkiss, of the Rockefeller Institute,

will make an address on "The Mode of Action of Gramicidin in Bacteria." Drs. Leonor Michaelis and F. Grannick, also of Rockefeller Institute, will read a paper on "Ferritin and Its Significance for Iron Metabolism."

Abraham White and T. F. Dougherty, of Yale University, will discuss "The Influence of Hormones on Serum Proteins." Research in "Physicochemical Characteristics of Albumins and Globulins of Human Plasma" will be reported by a Harvard group composed of Drs. L. E. Strong, J. L. Oncley, D. J. Mulford, W. L. Hughes, Jr., and E. J. Cohn. "Carbon Monoxide" will be the topic of a paper by F. J. W. Roughton, of Columbia University.

A report on "Clinical and Chemical Studies of the E Vitamins" will be presented by Dr. Philip L. Harris and K. Hickman, of Distillation Products, Inc. Dr. Roger J. Williams, of the University of Texas, will discuss "Studies on the Problem of the Significance of Folic Acid." Drs. T. F. Zucker, Lois Zucker and W. M. Sperry, of Columbia University, will speak on "Relative Growth" and on "The Distribution of Nitrogen, Fat and Water during the Life Cycle."

A second symposium to be held at the New York meeting will deal with sympathomimetic agents, compounds that affect the sympathetic nervous system of the body. It will be sponsored by the Division of Medicinal Chemistry, of which Dr. John H. Speer, of G. D. Searle and Company, Niles Center, Ill., is chairman. Papers will be read by Drs. Harry Gold, Cornell University Medical College, New York; Walter H. Hartung, University of Maryland; C. R. Scholz, Ciba Pharmaceutical Products, Inc., Summit, N. J.; M. L. Tainter, Winthrop Chemical Company, Rensselaer, N. Y.

SCIENTIFIC NOTES AND NEWS

THE eighth Edward Goodrich Acheson Medal and thousand dollar prize of the Electrochemical Society has been conferred on Dr. William Blum, chief of the Section of Electrochemistry of the U. S. Bureau of Standards, in recognition of his work for the standardization of electroplating methods and of plating formulas. The presentation will take place at the autumn convention of the society, which will open at Buffalo on October 13. Past recipients of the Acheson Award are: Edward G. Acheson, graphite and carborundum; Edwin F. Northrup, induction furnaces; Colin G. Fink, tungsten, chromium and tin; Frank J. Tone, silicon and silicon carbide; Frederick M. Becket, ferro-alloys; Francis C. Frary, aluminum; and Charles F. Burgess, electrolytic iron and dry batteries.

THE Borden Award, a gold medal and $1,000, for the most distinguished contribution to poultry science during the past year was presented at the recent annual meeting in Chicago of the Poultry Science Association to Dr. Fred R. Beaudette, poultry pathologist at the New Jersey Agricultural Experiment Station.

DR. BRUNO OETTEKING, curator of the department of physical anthropology of the Museum of the American Indian of the Heye Foundation, New York City, has been elected a corresponding member of the Sociedad Geografica of La Paz, Bolivia.

THE honorary degree of doctor of science was conferred at the commencement of the University of Maine on Dr. William H. Martin, dean of the College of Agriculture of Rutgers University.

THE University of St. Andrews has conferred the degree of LL.D. on Sir Robert Robinson, Waynflete professor of chemistry at the University of Oxford.

AT his request Dr. Ernst A. Bessey retired on July 1 from his work as dean of the Graduate School of the Michigan State College of Agriculture and Applied Science, a position that he has held since 1930. He is succeeded by Dr. R. C. Huston, dean of the Division of Applied Science. Dean L. C. Emmons, of the Liberal Arts Division, has been appointed dean of the School of Science and Arts, which has been formed by the merger of the Divisions of Applied Science and of Liberal Arts. Dr. Bessey continues as head of the department of botany.

DR. E. RAYMOND HALL, associate professor of vertebrate zoology and curator of mammals in the Museum of Vertebrate Zoology of the University of California at Berkeley, has a year's leave of absence to enable him to become professor of zoology, chairman of the department and director of the Museum of Natural History at the University of Kansas. He is taking the place of Professor H. H. Lane, who has reached the age of retirement for administrative appointees, but who will continue his work as professor of zoology.

DR. WILLIAM M. WHYBURN, chairman of the department of mathematics of the University of California at Los Angeles, has been elected president of the Texas Technological College. He has had leave of absence from the university for the past year to enable him to serve as a civilian officer with the operations analysis section of the Army Air Forces.

AT Indiana University Dr. Tracy Y. Thomas, since 1938 professor of mathematics at the University of California at Los Angeles, has been appointed chairman of the department of mathematics; Dr. Frank K. Edmondson has succeeded Professor W. A. Cogshall,

who recently retired as chairman of the department of astronomy. Vincent Nowlis, assistant professor of psychology at the University of Connecticut, has been appointed assistant professor of psychology. He will act as a research associate for studies on human sex behavior being carried out by Dr. A. C. Kinsey.

DR. JOHN S. LOCKWOOD, acting director of the Harrison department of surgical research of the School of Medicine of the University of Pennsylvania, has been appointed associate professor of surgery at the School of Medicine of Yale University.

DR. GRANVILLE A. BENNETT, of Tulane University School of Medicine, has been appointed professor of pathology and head of the department at the College of Medicine of the University of Illinois, Chicago.

DR. ALEXANDER THOM has been appointed to the professorship of engineering science at the University of Oxford.

DR. WILLIAM J. ROBBINS, director of the New York Botanical Garden, has been elected a member of the Board of Directors of the Boyce Thompson Institute at Yonkers, New York.

ELWOOD L. DEMMON, since 1928 director of the Southern Forest Experiment Station at New Orleans, has been named director of the Lake States Experiment Station at St. Paul, Minn. He succeeds Dr. Raphael Zon, director of the Lake States Station since 1923, who retires from active duty next month after forty-three years of continuous work with the Forest Service. Charles A. Connaughton, since June, 1938, director of the Rocky Mountain Station at Fort Collins, Colo., has been named director of the Southern Station.

J. KENNETH ABLEITER, principal soil scientist and chief analyst in soil uses and productivity of the Division of Soil Survey of the Bureau of Plant Industry, has been appointed chief inspector. In this position he is responsible for the system of soil classification, including the definition and nomenclature of the units of classification. He is succeeded as chief analyst by Dr. Carleton P. Barnes.

THOMAS P. FLEMING, medical librarian of the College of Physicians and Surgeons of Columbia University, has been appointed assistant director of the libraries of the university.

COLONEL GEORGE M. POWELL, M.C., director of the Special Planning Division, Operations Service, in the Office of the Surgeon General, who from 1939 to 1942 served as cardiologist and later became chief of the Medical Service at Gorgas Hospital, Ancon, Canal Zone, has been advanced from the grade of Lieutenant Colonel.

LIEUTENANT JOHN B. LUCKE, USNR, has leave of absence for military service from the University of Connecticut, where he is associate professor of geology and geography and head of the department. He has been assigned to the School of Photographic Interpretation, U. S. Naval Air Station, Anacostia, D. C., for further training until about October 1.

DR. LAWRENCE C. CURTIS, geneticist at the Connecticut Experiment Station at New Haven, has leave of absence for a year to serve on a food mission to North Africa with the Division of Relief and Rehabilitation of the Foreign Economic Administration to study the production of food crops in North Africa and the distribution of these crops to the Allies and to liberated countries.

DR. ALDO CASTELLANI, the well-known authority on tropical medicine, is one of twenty-five members of the faculty of the University of Rome who have been suspended. *The Times*, London, states that the majority of these men had either been ministers in the Fascist Government or had held high appointments in the corporations.

EROSION control on sandy soils and other soil conservation practices are being studied under auspices of the Government of Iceland by Páll Sveinsson, who is visiting in the United States. Previously he was associated for three years with the Soil Conservation Bureau under Gunnar Kristmundsson, who started the work some thirty years ago. Mr. Sveinsson plans to spend two or three years beginning this autumn at the University of Minnesota in studying agronomy, forestry and other subjects.

J. C. F. FRYER, who has been closely associated with the work of the British Agricultural Research Council since its establishment in 1931, has been released from his present work as director of the Laboratory of Plant Pathology of the Ministry of Agriculture and Fisheries to become secretary of the council. For the last three years he has been also acting as secretary of the Agricultural Improvement Council for England and Wales.

G. SHAW SCOTT has retired from the secretaryship of the British Institute of Metals, which he has held for thirty-six years. He is succeeded by K. Headlam-Morley, who will continue also as secretary of the Iron and Steel Institute.

THE National Metal Congress will meet in Cleveland during the week of October 16. W. H. Eisenman, national secretary of the American Society for Metals, is managing director. The congress is sponsored by the American Society for Metals in cooperation with the American Welding Society; the Iron and Steel and Institute of Metals divisions of the American Institute of Mining and Metallurgical Engineers; the

American Industrial Radium and X-Ray Society and the Society for Experimental Stress Analysis. Technical sessions will be held each morning, afternoon and evening. In addition to reports on research developments of the past year, there will be presented a series of practical panel-type production meetings each afternoon and evening, with the exception of Thursday evening when the annual dinner will be held.

THE first Institute of Dental Health Economics, sponsored by the School of Health of the University of Michigan, was held from June 26 to July 1 with an attendance of about a hundred. Twenty-six states were represented.

AN increase in quotas for admission to officer candidate courses leading to commissions in the Medical Administrative Corps of the Army, has been announced by the War Department. Quotas which until recently have been extremely limited have been revised to permit acceptance of 2,000 men within the next seven weeks for 17-week courses. The primary reason for the increase is the need for more officers qualified for administrative duties in the Army Medical Department to free members of the Medical Corps for professional duties. In recent months only the Medical Administration Corps Officer Candidate School at Camp Barkeley, Texas, has been accepting candidates. Under the new plan, the Officer Candidate School at Carlisle Barracks, Pennsylvania, has been reopened.

FIFTY-FIVE local governmental units in the Upper Peninsula of Michigan have availed themselves of the counseling service on post-war planning provided by the Michigan College of Mining and Technology. This service was first offered on July 1. Units which have received guidance include counties, all large cities in the Upper Peninsula, villages, townships, school districts and road commissions. The work falls under the Extension Division, and is conducted by Assistant Professor E. J. Townsend, a member of the staff both of the department of civil engineering and the department of economics. Professor Townsend has been designated by the Michigan State Planning Commission as its Upper Peninsula representative.

MR. AND MRS. MACO STEWART, of San Antonio and Galveston, recently offered their Galveston Island estate to the Medical Branch of the University of Texas for use as a convalescent home for children. The offer was accepted at the July meeting of the Board of Regents. The estate was named the Margie B. Stewart Convalescent Home for Children. Exceptional facilities are available for handling between thirty and fifty convalescent children, with outdoor fresh and salt water pools and special facilities for physical and occupational therapy. The project will comprise an important part of the Child Health Program of the Medical Branch as supported by the William Buchanan Foundation of Texarkana. The medical work of the new home will be under the direction of Dr. Arild E. Hansen, professor of pediatrics, while the surgical aspects and physical therapy will be under the direction of Dr. G. W. N. Eggers, professor of orthopedic surgery.

DISCUSSION

THE NATURE OF RH AGGLUTINATION REACTIONS[1]

THE question may be posed whether the special nature of the Rh reactions[2] is due to (1) some property of the Rh antigens, or (2) whether this can be traced to some special quality of the antibodies, or (3) whether peculiarities of both antigens and antibodies are responsible. Some observations by the authors have led us to the conclusion that the nature of the Rh antibodies is chiefly responsible for the peculiarities of the Rh reactions, though in addition special qualities of the agglutinogens may also play a rôle.

The anti-Rh sera most often used for diagnostic tests are human sera, obtained from mothers of erythroblastic babies.[3] The peculiarities of these sera are apparently due to the fact that they are *immune* sera, since they are produced in the mother in response to the stimulation of antigens of fetal origin, in contrast to the common blood grouping sera which are natural sera. This idea is supported by observations on human iso-immune sera against the properties A and B. (Such sera can be obtained from patients who have had transfusions of blood or plasma of an incompatible group, or from volunteer donors who have been given injections of small amounts of Witebsky's group substances A and B.) While most of these sera have approximately the same titer at room, body and refrigerator temperature, some have a much higher titer and react much more intensely and rapidly at body temperature than in the cold.[4] It is significant that these special human sera give less satisfactory reactions by the common slide technique at room

[1] From the Serological Laboratory of the Office of the Chief Medical Examiner of New York City. Aided by a grant from the United Hospital Fund of New York City.

[2] *Cf.* K. Landsteiner and A. S. Wiener, *Jour. Exp. Med.*, 74: 309, 1941.

[3] P. Levine, L. Burnham, E. M. Katzin and P. Vogel, *Am. Jour. Obstet. and Gynec.*, 42: 925, 1941.

[4] A. S. Wiener and R. B. Belkin, unpublished observations.

temperature than natural anti-A and anti-B sera of lower titer.

On the other hand, we have encountered a number of anti-Rh sera which give reactions comparable to those of the common grouping sera. These rather exceptional sera were of high titer, and were relatively insensitive to temperature variations, the reactions in some cases being even sharper in the cold than at body temperature. Such sera give good clumping by the common slide technique, and this we believe to be due to their wide temperature range of action. In addition, it may be mentioned that in the first report on the role of the Rh factor in hemolytic transfusion reactions Wiener and Peters[5] described post-transfusion sera which had the peculiarity that they reacted only in the cold.

In this connection, it would seem that agglutinins should be named according to their specificity rather than the optimum temperature at which they react. The recent tendency of some workers to designate autoagglutinins merely as cold agglutinins is therefore to be condemned. The term autoagglutinins is unambiguous and therefore should be retained, though if one wishes, one may use the expression "cold autoagglutinins" to indicate that the reactions are most distinct or occur only in the cold.

As already mentioned we believe that in addition to the special properties of the Rh antisera certain peculiarities of the agglutinogen may play a part in the nature of the reactions. Even with the most potent Rh antisera, in tests at body temperature, isohemolysis has rarely, if ever, been observed in the test-tube. Moreover, the Rh agglutination reactions are more easily reversed by shaking than in the case of the common grouping tests. Finally, only about 2 per cent. of Rh negative individuals respond to transfusions of Rh positive blood by the production of isoantibodies, while A and B appear to be regularly antigenic in man. One of us (W)[6] has previously suggested that the reason for this may be that Rh is a sub-surface antigen, while A and B may be located at the surface of the erythrocyte. Recent studies[7] on the capacity of red-cell stromata to inhibit anti-Rh as well as anti-A and anti-B sera, indicate that a more likely explanation may be that there are far fewer Rh hapten groups than A and/or B hapten groups per erythrocyte. This hypothesis would account for the peculiar *in vitro* behavior of the Rh tests as well as the apparently poor antigenic action of the Rh factors. If the hypothesis proves to be correct, then this would be a serious obstacle to attempts at extraction of large amounts of Rh antigen from human erythrocytes for clinical use.

ALEXANDER S. WIENER
EVE B. SONN
RUTH B. BELKIN

[5] A. S. Wiener and H. R. Peters, *Ann. Int. Med.*, 13: 2306, 1940.

[6] A. S. Wiener, "Yearbook of Pathology and Immunology for 1941," p. 499.

[7] A. S. Wiener and R. B. Belkin, unpublished observations. This effect would be exaggerated if the testing serum contains "blocking" antibodies (A. S. Wiener, *Proc. Soc. Exp. Biol. and Med.*, June, 1944), which would further reduce the number of sites available for attachment of anti-Rh agglutinins.

USE OF TERMS RELATING TO VEGETATION

PLANT-COVER data are commonly added to military maps for various theaters of war. Members of our armed forces in many countries quickly learn to distinguish hitherto strange types of vegetation, as they appear from the air as well as on the ground. Investigators of economic plants are rapidly adding to our knowledge of vegetation of Latin America. This recently increased awareness of vegetation is unprecedented. Suggestions toward accurate use of names for certain vegetation-types should be timely.

"Vegetative cover" is frequently used by many workers in applied ecology, by some geographers, and others. The only objection to it is the long-sanctioned use in biology of "vegetative" to contrast with "reproductive." Vegetative as applied to herbage or other plant cover thus introduces an irrelevancy. This is easily avoided by substituting "vegetation" or "plant cover." When an adjective derived from vegetation is desired, vegetational or the rather rare word "vegetal" may be used.

Terms such as prairie, meadow, savanna, woodland, scrub forest, evergreen forest, etc., convey distinctive impressions of appearance or physiognomy of vegetation, without commitment as to the plant species which make it up. Unfortunately the impressions are not the same for all persons, largely through limited knowledge of the history and applications of particular terms. Thus "savanna" was first applied to extensive grasslands in the West Indies, and is now most commonly so used there and in central and northern South America. In some of these savannas single trees or small clumps or groves of trees, or thickets of shrubs, occur. The proportion of area occupied by taller woody vegetation, and its composition, are highly variable. This accompaniment of savanna trees and shrubs, though incidental, has led some travelers or readers of travels to consider that savannas necessarily are tree-and-grass combinations, synonymous with "parkland." The primary significance of the grassland, by itself or serving as a matrix for trees, should be kept in mind. A two-phase vegetation should be named after both phases, as prairie with hazel thickets, or palm savanna (*i.e.*, savanna with palms). Tendencies to extend the term savanna to

cover vegetation-types in higher latitudes or in very arid country should not be followed.

Another commonly misused word is *scrub*. Some have considered that a vegetation of shrubs is a scrub. This is true only if the shrubs are scrubby (in the usual connotation of sparse, dwarfed or malformed). Scrubby trees and scrubby bushes likewise are prevalent in certain areas. *Shrub*, as long advocated by Dr. H. L. Shantz, is a valid designation for cover made up of shrubs of normal stature and form. Scrub forest is composed of reduced and perhaps gnarled trees. Scrub woodland is an open cover of such trees.

Though a significant distinction between shrub and bush is not usually emphasized, it is frequently implied, and can be usefully employed, for vegetation as well as for growth-form of particular plant species. Most shrubs are larger than bushes, with relatively few, sizable, hard-wooded, long-lived stems. Bushes average smaller, with many stems which are slender. In many bushes the stems have large pith, little wood and are individually short-lived. Most Mediterranean maqui is evergreen shrub (with or without tree components in addition), whereas garrigue cover-types are chiefly bush (in the sense of bush vegetation). The corresponding American vegetation, sometimes called coastal sagebrush, might better be known as California bush, since it is far more extensive than the Artemisia species after which it was named and comprises over a hundred bush species.

There is considerable disagreement as to application of "chaparral." It has been used for a mixed shrub at the east front of the Rocky Mountains, for shrub of Nebraska sand hills, and for shrub mixtures in the eastern states. Texas botanists may rightly claim that mesquite shrub of the southwestern states has been called chaparral for nearly a century. (Chaparral of Texas is so defined in J. R. Bartlett's "Dictionary of Americanisms" in 1850.) But if so widely used, chaparral becomes merely a synonym of shrub vegetation. Several botanists have suggested that clarity will result if students of vegetation can agree to restrict the term to evergreen shrub. Most variants of this evergreen shrub are xerophytic rather than of types usually associated with forest. Chaparral is not limited to California nor to climates with rainfall only in winter; it occurs in its usual appearance but with fewer species, in mountains of Arizona, New Mexico and southwestern Colorado; and chaparral types with species of the usual genera occur in several parts of Mexico. If describers of deciduous shrub will refrain from calling it chaparral, the latter word can be usefully preserved. Mesquital or simply mesquite should be distinctive for Texas vegetation dominated by this one plant.

Attempts to visualize just what may be meant by "jungle" in accounts by various authors, usually result in confusion. To some minds jungle is synonymous with tropical rain-forest of Asia and the East Indies or with any tropical forest. From its first meaning as "waste or uncultivated ground (='forest' in the original sense)" (Oxford Dictionary) it has come to be applied to a wide variety of vegetation-types: brushwood, long grass, any tangled vegetation, "low or thin forest" (J. C. Willis), primeval forest, secondary forest. Many foresters and botanists working in India use the term rarely or not at all. The clearest statement found is by T. W. Webber:[1] "Dr. Johnson defines a forest as a 'wild uncultivated tract of ground interspersed with trees.' . . . In India the term 'jungle' has a similarly wide and uncertain meaning, not necessarily implying trees any more than the Scotch 'deer forest,' but signifying a region where savage animals dwell, and where wild men exist." Current wide use of the phrase "jungle warfare" should not lead followers of the news into picturing any one type of vegetational setting for the fighting in Burma or New Guinea or islands of the western Pacific. When a particular kind of vegetation is to be described, the word "jungle" is of no value. Use of this term for regions other than Indomalaya should be avoided in writings on science.

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NOTES ON "A FLOWMETER FOR USE IN AIR SAMPLING PROCEDURES"

I HAVE read the article "A Flowmeter for Use in Air Sampling Procedures," by H. M. Lemon and H. Wise, in the issue of SCIENCE for January 14. Industrial hygienists have used similar flowmeters for several years, and in this laboratory we consider this type of device as very commonplace. In our classes in industrial air analysis we emphasize the importance of calibrating the flowmeter with the sampling device when the flowmeter is placed downstream. The use of the flowmeter downstream of the air sampler has decided advantages in fume and gas sampling where the resistance of the sampler remains constant, as it eliminates apparatus to be cleaned and a source of error. It is difficult to clean certain metal fumes from the flowmeter placed upstream even with highly corrosive acids because of the adhesiveness of the fume particles.

A commercial air-sampling instrument for carbon monoxide (M.S.A. CO. Indicator, manufactured by Mine Safety Appliances Company), which has been marketed for over fifteen years, has a simple flowmeter of this type preceding the dehydrating canister and absorption chambers. The change in resistance of the dehydrating canister with absorption of moisture therefore does not affect the calibration.

1 "The Forests of Upper India," p. ix. London, 1902.

It is not necessary to make 120 determinations in the calibration of the device or any orifice or capillary flowmeter. Since the pressure drop across the orifice is an exponential function of the air flow, a log-log plot will yield a straight line which simplifies the number of points necessary for accurate calibration. Theoretically two checked points (four determinations) at the upper and lower limits of the desired range will fix the calibration; however, it is customary to make a further check by checking a third point at an intermediate air flow.

The device is also limited in making accurate samples of air from flow in ducts or pipes, since its entrance is too short. For accurate duct or pipe sampling it is customary to traverse the cross-section of a duct since the air flowing at the walls of the duct is not moving at the same rate as portions near the centerline. It is also necessary in accurate duct sampling to have the inlet velocity to the sampling tube at the same air velocity as the sampled air stream. The authors do not mention the necessity for this and fail to provide a long enough inlet to the flowmeter for duct traversing.

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SCIENTIFIC BOOKS

THE CHEMICAL PROCESS

Chemical Process Principles. Part I: *Material and Energy Balances.* By O. A. HOUGEN and K. M. WATSON. 452 pages. New York: John Wiley and Sons. $4.50.

THE reviewer desires to present the reactions of a professor of physical chemistry to this volume, the first part of a text-book of chemical engineering which deals with the principles underlying chemical processes as carried out in industry. Oftentimes our chemical engineering colleagues complain about the matter which the physical chemists present to students in the physical chemistry courses basic to chemical engineering curricula. A perusal of the present volume has provided one teacher of physical chemistry with an understanding of the factors underlying such complaints.

The volume presents, in some 450 pages and ten chapters, approximately ten topics including stoichiometry, ideal gases, vapor pressures, partial and complete saturation, solubility, distribution, sorption, thermophysics, thermochemistry, fuels and combustion and material and energy balances in important industrial processes. The physicochemical principles corresponding to these topics normally occupy approximately one fifth of a full-year course in physical chemistry. The authors of this text state, however, that the material of this first part of their planned complete text "is suitable for second- and third-year undergraduate work." A portion of the as yet unpublished second part "is suitable for third- and fourth-year undergraduate work; the remainder is of graduate level." Here then is the secret of the dissatisfaction sometimes expressed concerning fundamental courses in physical chemistry; the time available to the professor of physical chemistry is much too small to secure the desired result in the training of chemical engineers.

This present text indicates excellently the reasons why the standard physical chemistry course is inadequate for chemical engineering curricula. It shows that courses in chemical process principles resolve themselves into term or year courses in the solution of problems based upon the fundamental principles which the physical chemist can do little more than outline, can, at best, employ one hour per week in the discussion of such applications. It is a text, however, which every professor of physical chemistry would do well to have close at hand. It will help him to emphasize the topics which many of his students will later have to master in the thoroughgoing fashion that this book requires of its readers. It will be a source book for better problem work than the physical chemist normally requires of his students. It will give him a more sympathetic understanding of the labors of his chemical engineering colleagues that go into the development of the chemical engineering fraternity now so largely responsible for the high technical level of American chemical industry. The students who master this text are assured of an excellent introduction to the process problems of chemical engineering. They will go forward with a thorough preliminary training to the more difficult thermodynamic and kinetic phenomena that are promised in Part 2 of this text. In spite of war conservation regulations, the format of the book is pleasing. The right half of equation 2, page 60, is initially confusing. Some of the thermochemical data on pages 262 and 263 and in the following pages convey an idea of greater accuracy than is probably warranted by the data themselves. Perhaps kcal. should be used in several places instead of cal., for example, on page 273 but not as on page 310. The illustrations are excellently legible and illustrative. The problem illustrations are well presented and the problems to be solved abundant and well chosen.

HUGH S. TAYLOR

PLANTS OF A CALIFORNIA MOUNTAIN

The Flowering Plants and Ferns of Mount Diablo, California. By MARY L. BOWERMAN. xi + 290 pp. 6 × 9 in. Berkeley, California: The Gillick Press. 1944. $3.75.

THIS study of the vegetation of a 3,849 foot mountain lying 30 miles east-northeast of San Francisco is a model local flora. It begins with brief accounts of the geography, climate, soils, geology and paleobotany of the area. Then there is a full and well-illustrated discussion of the ecologic relations, the vegetation being classified into five major formations, subdivided into associations and in some cases lesser categories. Succession, especially in relation to overgrazing and fire, is also treated. Only slight correlation between substratum and vegetation can be traced.

The vascular plants thus far observed number 630 species, 91 of them introduced and 6 endemic. The geographic relations of these are discussed, and a list is given of species which in view of their occurrence in neighboring regions would be expected on Mt. Diablo, but have not as yet been found there. A history of botanical exploration in the area follows, with an account of the 25 entities named on the basis of specimens collected here.

In the annotated catalogue there are extensive keys; the plants are arranged in the Engler-Prantl sequence, with nomenclature according to the International Rules of 1930. For each entity are given the habitat, associated plants, localities and miscellaneous interesting data. Common names are supplied for many species, mostly locally used or descriptive ones; for instance, *Viola sheltoni,* which in "Standardized Plant Names" became automatically "Shelton Violet," is here assigned the more significant name of "Fan Violet."

In this connection a few minor criticisms occur to the reviewer. The keys are rather over-simplified, and not entirely "fool-proof." Thus, the *Polemoniaceae,* being a somewhat baffling group, is rarely covered in manuals and floras without some mistakes or misunderstandings; and the present work is no exception, for when one runs down the opposite-leaved members, constituting half the family here, in the key on page 86, they fall into *Gentianaceae.* Then, in certain instances the taxonomy is antiquated, as when the California Trillium is classed as *"T. sessile* var. *giganteum,"* a long-discredited case of Hookerian lumping. In general, more synonymy might well have been given to aid the outsider in interpreting the names adopted.

The book closes with a bibliography, a glossary and an index to technical and common names. Well printed on superior paper, it constitutes a most creditable piece of work.

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NEUROANATOMY

Human Neuroanatomy. By OLIVER S. STRONG and ADOLPH ELWYN. Pp. 417. Baltimore: Williams and Wilkins Co. 1943. $6.00.

THE arrangement of material in this text-book is well suited to the presentation of human neuroanatomy in the lecture room and particularly in the laboratory. No important topic is omitted, and each is kept in its proper proportion to the whole. Two of the good points of this book are the close integration of topics and brevity of presentation of fundamentals. The reading matter is accurate, thought-provoking and informative.

There is a well-considered introduction, followed by developmental anatomy and histological structure of the nervous system, including the spinal ganglia, terminations in receptors and effectors, meninges, nerve cells, etc. In the body of the book the spinal cord and the segments of the brain stem are considered in some detail. The book ends with consideration of the cerebral hemispheres and cerebral cortex. There is a selected bibliography and an index.

This is not a book that can be read wholly without reference to laboratory material. In fact, one of its virtues is that the text serves to motivate the earnest student to look more closely into the structure of the nervous system as revealed by sections of the spinal cord and brain stem. It can serve admirably as a laboratory text, and will relieve to a measurable extent the burden of individual instruction.

The illustrations are for the most part excellent and well chosen. There are topical guides to the central connections of the cranial nerves and to cross-sections of the brain stem. The labeling of structures is simplified, well-done, following conventional terminology. Besides human material little else is introduced. The illustrations are quite suitable, since many laboratories are stocked with similar preparations.

The doctrines of neural levels and systems have been carried out chapter by chapter in the body of the book to the extent that some of the more elementary conduction processes can be assigned to anatomic systems. There is the usual difficulty of carrying each afferent system from receptor organs through various central levels. One gets the impression that a number of additional diagrams of systems might enhance the value of the text. The dorsal thalamus and its cortical connections might well have been separated from the basal ganglia and motor apparatus. Clinical applications are too scanty to satisfy the practically minded student. They will in many instances have to be supplied from other sources.

There is only a modest attempt to treat the brain as an organ for turning out physiological products related to acquisitive functions and achievement. In this respect the dorsal thalamus and cerebral cortex

suffer most. The book, in common with other texts, does not supply an anatomical organization underlying the elaboration of the acquisitive and actional functions which play such a large part in the biologic activity and achievements of the individual.

There are a few minor errors in the book. On the whole the student as well as the teacher will find it a useful text.

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GENES AND THE MAN

Genes and the Man. By BENTLEY GLASS. 386 pp. The Science in Modern Living Series. Teachers College, Columbia University. 1943. $3.50.

ONE of the most encouraging phenomena of our time is that, as Professor Glass says, "throughout the realm of science the narrow, rigid boundaries of specialized fields of subject-matter are at last breaking down. The boundary between genetics and cytology has already disappeared, and it is now evident that embryology and physiology are beginning to enter the amalgam." This volume is not intended to be a new text-book of genetics. Rather, it "has been prepared to indicate a new outlook," namely, "that we should understand the epic sweep of an individual's growth and development up to maturity and the long years of slow decline thereafter, together with those tenuous bonds that link each generation with all before and after . . . by tracing them from their beginnings in protoplasm and the genes." This is a very ambitious undertaking, and considering its novelty and magnitude Professor Glass has mastered the task quite well.

The presentation begins with a discussion of the possibilities of spontaneous generation, of viruses, cell structures, cell division and of elements of cellular physiology. The concept of genes is introduced without reference to Mendelian heredity. In Chapter II we have a description of sex cells, fertilization, meiosis, mutation, Mendelism, linkage and crossing over. On page 118 a gene is defined as "a single member of the linear series of hereditary factors within each chromosome. Its unitary nature is defined by its separability from its neighbors through crossing over." The reviewer is afraid that such a definition may give comfort to those who doubt the existence of genes. A very good account of the genetic basis of sex is found in Chapter III; Chapter IV combines discussions of gene interactions, gene effects in development, embryonic induction, sex hormones, heterogenic growth and the nature-nurture problem. Chapter V is the longest, as it may well be, since it presents a condensed and yet very readable account of human embryology with excursions into comparative anatomy, physiology and endocrinology. The final chapter is concerned with biological aspects of vital statistics and physiology of ageing.

In a book which sets out "to describe the operation and interaction of those factors which make the physical man" one expects to find a discussion of man's evolution and of genetic evolutionary mechanisms, but these topics are almost completely ignored. However, this complaint may not be a fair one, for even as it stands a tremendous amount of diversified information is condensed between the covers of this medium-sized volume. At times one wishes either that some of the less essential information were removed to give a greater prominence of fundamentals, or else that the book were expanded much beyond its present size. In any case, Professor Glass must be congratulated with having produced a new and interesting type of book on general biology.

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SPECIAL ARTICLES

CONTROL OF GRAM-NEGATIVE BACTERIA IN EXPERIMENTAL ANIMALS BY STREPTOMYCIN[1,2]

STREPTOTHRICIN, an agent isolated from a soil Actinomyces, was found[3,4] to be effective against certain gram-positive bacteria, as well as against a variety of typical rod-shaped gram-negative bacteria, not only *in vitro* but also *in vivo*. These results were recently confirmed.[5] However, the action of streptothricin upon other gram-positive bacteria, such as *Bacillus mycoides,* and upon some gram-negative bacteria, such as *Pseudomonas fluorescens, Ps. aeruginosa, Proteus vulgaris* and *Serratia marcescens,* is rather limited. Recently, another antibiotic agent, streptomycin, was isolated[6] and found capable of acting upon these bacteria as well; otherwise, it resembles streptothricin in its chemical behavior and mode of action. This agent has been found to be active against various gram-negative bacteria also in the animal body.

[1] Journal Series paper of the New Jersey Agricultural Experiment Station, Rutgers University, Department of Microbiology.

[2] With partial support from a grant made by the Commonwealth Fund of New York.

[3] H. J. Metzger, S. A. Waksman and L. H. Pugh, *Proc. Soc. Exp. Biol. Med.,* 51: 251, 1942.

[4] H. J. Robinson, "Some Toxicological, Bacteriological and Pharmacological Properties of Antimicrobial Agents Produced by Soil Microorganisms." Thesis, Rutgers University, 1943.

[5] H. J. Robinson, O. E. Graessle and D. R. Smith, SCIENCE, 99: 540, 1944.

[6] A. Schatz, E. Bugie and S. A. Waksman, *Proc. Soc. Exp. Biol. Med.,* 55: 66, 1944.

The preparation of streptomycin used in these studies was still in a fairly crude state, having about 30,000 dilution units[7] per gram of crude dry material. The toxicity of this preparation was LDO = 35 mg and LD100 = 135 mg per 20 gm mouse, when injected intraperitoneally. The results of a typical experiment on the effect of streptomycin upon *Salmonella schottmülleri* are presented in Table 1. 6.4 mg of the material, equivalent to a total of 190 units, was sufficient to give complete protection to mice weighing 18–20 gm. The effect of streptomycin upon *Pseudomonas aeruginosa,* an organism known to be highly resistant to the antagonistic action of other bacterial or of antibiotic substances, since it itself produces two active antibacterial agents, is brought out in Table 2.

TABLE 1

ACTIVITY OF STREPTOMYCIN IN MICE INFECTED WITH S. SCHOTTMULLERI

No. of mice	Treatment	Dilution of bacterial culture used for infection	Survival of mice, in hours					
			18	24	30	42	48	72
5	Control*	10^{-5}	2	0	0	0	0	0
3	Control	10^{-6}	1	0	0	0	0	0
3	Control	10^{-7}	1	0	0	0	0	0
3	Control	10^{-8}	2	0	0	0	0	0
5	Streptomycin, 6.4 mg†	10^{-5}	5	5	5	5	5	5
5	Streptomycin, 12.8 mg†	10^{-5}	5	5	5	5	4	4

* No streptomycin.
† Divided in 4 doses, every 6 hours.

TABLE 2

ACTIVITY OF STREPTOMYCIN IN MICE INFECTED WITH PS. AERUGINOSA

No. of mice	Treatment	Dilution of bacterial culture	Survival of mice, in hours					
			18	24	30	42	72	168
5	Control	10^{-4}	2	0	0	0	0	0
3	Control	10^{-5}	2	0	0	0	0	0
3	Control	10^{-6}	2	3	2	2	0	0
3	Control	10^{-7}	3	2	2	2	2	2
5	Streptomycin, 6.4 mg*	10^{-4}	4	3	3	0	0	0
5	Streptomycin, 12.8 mg	10^{-4}	5	4	3	1	1	1
5	Streptomycin, 6.4 mg	10^{-5}	5	5	5	5	5	4
5	Streptomycin, 12.8 mg	10^{-5}	5	5	5	5	5	4

* Divided into 4 doses, injected every 6 hours.

The addition of 6.4 to 12.8 mg of streptomycin per mouse gave excellent protection against infection by this organism as well.

For the study of the effect of streptomycin upon fowl typhoid (*Shigella gallinarum*), 11-day-old chick embryos were used. The first four preliminary experiments gave rather striking results; all the controls died from the infection, whereas the greater number of embryos treated with streptomycin were protected. One of the difficulties encountered in these experiments was the fact that the embryos were readily injured by attempts made to give repeated injections, and many of them died from causes other than the infecting organism. The results of such a typical experiment are given in Table 3. A 24-hour broth culture of *Sh. gallinarum,* centrifuged for 12 minutes, at 750 r.p.m., was used. The bacterial count was 88×10^8 per milliliter. One-tenth ml portions of two dilutions of the bacterial preparation were used for infecting each embryo. The eggs were further incubated for 12 days, and the chicks remaining alive were killed. Cultures were made from the heart and liver of both the dead and the killed embryos and the hatched chicks. The results prove emphatically that streptomycin, if used in sufficient concentration, offers full protection to chick embryos against fowl typhoid, even the dead embryos showing complete freedom from the disease.

TABLE 3

ACTIVITY OF STREPTOMYCIN IN CHICK EMBRYOS INFECTED WITH FOWL TYPHOID

No. of eggs	Treatment	Dilution of bacterial culture	Survival of embryos in days				Died from fowl typhoid
			2	4	7	12	
3	Control	10^{-6}	3	1	0	0	3
3	Control	10^{-5}	3	0	0	0	3
5	Streptomycin, 5 mg	10^{-6}	4	3	2	2	1
5	Streptomycin, 5 mg	10^{-5}	5	5	4	3	0
5	Streptomycin, 10 mg	10^{-5}	5	4	3	0	0

Finally the results of an experiment on the effect of streptomycin upon *Brucella abortus* may be reported here. Fifteen-day-old chick embryos were used. A relatively small number of bacterial cells were injected into each embryo. The streptomycin was a crude liquid preparation containing 100 mg of dry, ash-free material per milliliter. Against this pathogen as well (Table 4), excellent protection was afforded by streptomycin.

TABLE 4

EFFECT OF STREPTOMYCIN ON BRUCELLA ABORTUS IN CHICK EMBRYOS*

Total number of embryos	Treatment	Presence of *Brucella abortus* in embryos†	
		Membrane	Liver
5	Control, untreated	4	3
6	Streptomycin, 5 mg	2	0
6	Streptomycin, 10 mg	0	0
6	Streptomycin, 20 mg	0	0

* 4,000 cells of *B. abortus* injected into each embryo.
† The tests for the presence of *B. abortus* were made on the death of the embryo or on the 21st day of incubation of the eggs.

Complete protection in experimental animals was also obtained against *Proteus vulgaris.*

The authors are indebted to Dr. F. R. Beaudette, of the Poultry Department, for making the facilities of his laboratories available for the tests on *Shigella,* and to Dr. H. Robinson and Miss D. Smith, of the

Merck Institute of Therapeutic Research, for making some of the tests presented in this paper.

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IN VITRO FERTILIZATION AND CLEAVAGE OF HUMAN OVARIAN EGGS[1,2]

FIRST stages in the cleavage of the fertilized human egg have, as far as we know, never been reported, and while *in vitro* fertilization of tubal eggs of the rabbit has been described,[3] we have found no record of such experiments in higher mammals. A monkey egg fertilized *in vivo* has been cultured *in vitro* from the two- to the eight-cell stage.[4]

Utilizing the surgical material available at the Free Hospital for Women, we have, during the past six years, made numerous attempts to achieve *in vitro* fertilization and cleavage of human eggs obtained from ovarian tissue removed just prior to the expected time of ovulation. Throughout this period of investigation,[5] several factors have been varied; *e.g.*, the conditions of culture of the eggs, both before and after exposure to spermatozoa, the duration of contact of egg and spermatozoa, and the concentration of the sperm suspensions employed.[6] As a result of recent modifications of our method, we believe we have succeeded in three experiments, which constitute the subject-matter of the present report.

In two of these cases (D. D. and R. P.), the egg, after being subjected to certain procedures (to be described later), was found to be in the two-cell stage. In the third case (J. D.), two eggs divided. One of these, when first seen in cleavage, consisted of one large blastomere and two smaller ones, each of the three containing a round, vesicular nucleus. The second egg from this same patient was in a similar stage, but part of the cytoplasm appeared fragmented, and soon proceeded to undergo rapid degenerative changes. In this first report, we will confine our discussion, therefore, to the two eggs in the two-cell stage and the more normal of the two eggs in the three-cell stage.

THE TWO-CELL STAGE OF THE HUMAN EGG

The first specimen was obtained from Mrs. D. D. (No. 20,768), a 38-year old Para IV, who underwent laparotomy on the 10th day of her menstrual cycle, at a time when the endometrium was in the early proliferative stage. When first observed in the fluid drained from a 2.3-cm bluish follicle, the egg was enclosed within a moderate investment of granulosa cells. It was washed in Locke's solution and incubated for 27 hours in the serum of the same patient. Then it was exposed for one hour at room temperature to a washed sperm suspension in Locke's solution. The watch-glass containing the ovum and spermatozoa was left on the stage of the dissecting microscope and the egg was kept in constant view (at a magnification of $\times 35$). The spermatozoa showed great activity throughout the period of observation; they were clearly seen to travel through the interstices of the loose cellular formation surrounding the egg, and many were noted in active motion just outside the ovular boundary. At the end of one hour, the ovum was transferred to fresh serum from a post-menopausal patient. As the egg was pipetted into the culture flask, the cellular investment suddenly dropped off and it appeared as a single round cell with a fuzzy border. When it was again observed after 40½ hours' culture, it was found to consist of two blastomeres, each measuring 86 μ in diameter, and was enclosed within a zona pellucida of uniform width, measuring 14 μ. A sketch of the specimen was made and it was fixed in Bouin's solution, but in the lengthy process of dehydration, it was, unfortunately, lost.

A stained section of the follicle from which this egg was obtained showed a typical preovulatory phase.

Of the second egg in the two-cell stage we have a complete series of stained sections. Essentially the same procedure, as described above, was carried out on an ovum, washed out of a follicle of Mrs. R. P. (No. 14,518), a 31-year old Para VI, Gravida VIII, who was operated upon on the 11th day of her cycle. The endometrium at this time was in the early to mid-proliferative phase of its development.

Thirteen eggs in all were recovered from the follicles

[7] A unit of streptomycin is that quantity of the antibiotic agent which inhibits the growth of a given strain of *Escherichia coli* in 1 ml of nutrient broth or agar.

[1] From the Department of Gynecology, Harvard Medical School, Boston, and the Fertility Clinic Laboratory, Free Hospital for Women, Brookline, Mass.

[2] Aided by grants from the William F. Milton Fund of Harvard University, the Committee for Research in Problems of Sex, National Research Council, and the Carnegie Corporation of New York.

[3] G. Pincus and E. V. Enzmann, *Proc. Nat. Acad. Sci.*, 20: 121, 1934.

[4] W. H. Lewis and C. G. Hartman, *Carnegie Inst. Wash. Pub. 443, Contrib. to Embryol.*, 24: 187, 1933.

[5] Nearly 800 human follicular eggs have been isolated and studied during the course of this investigation; of these, 138 have been observed after exposure to spermatozoa.

[6] We very gratefully acknowledge the invaluable advice and encouragement generously given us by Dr. Gregory Pincus, as well as the helpful assistance furnished at various stages by Dr. Nicholas T. Werthessen, Miss Lotte Lee Sichel, Miss Eleanor C. Adams, James M. Snodgrass and Dr. Harold Brown. We are also deeply indebted to Dr. Austin M. Brues for his help in the early part of this investigation, and to Dr. Arthur T. Hertig for his constant encouragement, advice and material aid through his grant from the Carnegie Corporation of New York.

of this patient and were cultured in three batches. The egg to be described was one of a set of four, of which three, when first seen, were covered by a thick granulosal cell investment, and one by only a few rows of cells. The eggs were incubated in serum[7] for 22½ hours, being washed in salt solution before and after incubation, and then exposed to a washed sperm suspension in Locke's solution for two hours at room temperature. They were again washed in Locke's solution and cultured in fresh serum for 45 hours.

When examined at the end of the incubation period, one egg was found to be in the two-cell stage; it resembled very closely the first specimen described. Two blastomeres of fairly uniform size and appearance, and containing granular cytoplasm, were enclosed within a zona pellucida along the border of which were numerous spermatozoa. At least one of them was clearly seen within the zone. The entire egg (including the zona pellucida) measured 153 μ × 155 μ; the cellular portion was 100 μ × 113 μ, and the average width of the zona pellucida was 23 μ. The blastomeres measured 88 μ × 58 μ, and 105 μ × 58 μ, respectively.

The egg was fixed in a plasma clot according to the method described by Pincus,[8] and the clot was carried through the double embedding celloidin-paraffin method, serially sectioned at 8 μ, and stained with hematoxylin and eosin. The specimen is included in 8 sections; hence the total thickness of the egg is approximately 64 μ. In a section through the middle of the specimen, the blastomeres (designated for convenience as "A" and "B") measure 63 μ × 39 μ and 66 μ × 36 μ, respectively. The cytoplasm appears uniformly granular, with the exception of the polar regions, where there is beginning vacuolization, as had been noted in the fresh specimen just prior to fixation. In the approximate center of each cell there is a round, vesicular nucleus measuring 18 μ × 13 μ, and 16 μ × 15 μ (blastomeres "A" and "B," respectively), and containing a chromatin meshwork. The zona pellucida surrounds the egg over about two thirds of its circumference. The failure to retain the entire zona pellucida in section was doubtless due to the method of fixation, as it had been intact in the fresh specimen. In its widest portion, the zona measures 7 to 8 μ in width. At least 4 sperm heads may be identified; one of them appears to be just within the cell body of blastomere "B." In a section adjacent to the one just described, a polar body measuring 18 μ × 10 μ is seen beside blastomere "A." It contains what appears at first as a more or less definite number of chromatin units discrete enough to be counted. However, upon further magnification, the chromatin is seen to be in the form of a lobulated body, only two clumps being definitely separated from the general mass. Opposite blastomere "B" at the outer boundary of the zona, two sperm heads may be identified. Other sections of the egg, devoid of nuclear material, show 1, 5, 7 and 9 spermatozoa, respectively, in the neighborhood of the zona pellucida or of the cytoplasm itself.

The Three-Cell Stage

The third experiment to be reported was performed on ova of Mrs. J. D. (No. 21,012), a 38-year old sterility patient, in whom the diagnosis of tuberculous endometritis had been made after routine biopsy taken in the course of an investigation for sterility. Operation on the 12th day of her cycle, at a time when the endometrium was in the late proliferative stage, revealed extensive tuberculous involvement of the uterus and tubes. Both tubes were sealed and the fimbriae were inverted.

Two out of four eggs, recovered in washings of incised follicles and subjected to essentially the same procedures as outlined above, were found to be in cleavage. These ova had been cultured in serum for 27 hours prior to contact with spermatozoa, exposed to the latter for one hour and ten minutes, and reincubated for 46 hours. At the end of the incubation period, the more normal of the two specimens consisted of three well-defined, round, regular blastomeres, each of which contained a round, vesicular nucleus. A photograph taken two hours later already shows beginning signs of degeneration; *i.e.*, shrinkage and vacuolization. Within the zona pellucida, which is of even width, at least 5 spermatozoa are seen. The entire egg (including the zona pellucida) measured 170 μ × 183 μ; the cellular portion was 103 μ × 127 μ, and the zona pellucida averaged 21 μ in width. The largest blastomere measured 97 μ × 73 μ, and the two smaller ones 62 μ × 62 μ, and 50 μ × 63 μ, respectively.

The ovum was fixed, serially sectioned, and stained in the same manner as the second egg, described above. Since it includes 10 sections, cut at 8 μ, the specimen is approximately 80 μ thick. A section through the middle measures 50 μ × 86 μ. The largest blastomere is here seen to be 66 μ × 49 μ, and the two smaller ones, 35 μ × 38 μ and 33 μ × 44 μ, respectively. In addition to vacuolization of the cytoplasm, the presence of multiple nuclei in the individual cells is evidence that the egg had undergone definite degenerative changes since it was first observed in cleavage that afternoon and sketched.

In one section there is a structure measuring 14 μ × 9 μ, which is strongly suggestive of a polar

[7] In all three experiments reported here, the serum used for culture of the eggs prior to exposure to spermatozoa was taken from the patient who had furnished the eggs, while subsequent culture (following contact with spermatozoa) was carried out in serum of a post-menopausal patient.

[8] G. Pincus, *Jour. Exp. Zool.*, 82: 85, 1939.

body. Nowhere throughout the preparation is there any sign of the zona pellucida; this had evidently been dissolved by the fixative.

In regard to the duration of early cleavage stages, it is pertinent to cite the report of Lewis and Hartman[4] on the culture *in vitro* of the monkey egg fertilized *in vivo*. They state that in their experiment, in which fertilization was believed to occur soon after ovulation, the one- and two-cell stages lasted at least 36 hours. We observed two eggs in the two-cell stage 40½ and 45 hours, respectively, following contact with spermatozoa. Lewis and Hartman considered that the three- and four-cell stages in their egg extended to the 48th hour following fertilization. Our two eggs were seen in the three-cell stage 46 hours after exposure to spermatozoa. Hence, our findings, in this respect, are in general agreement with those reported for the monkey egg.

These experiments will be described in greater detail elsewhere, and photographs of the fresh and fixed specimens will be included.

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THE PROLONGATION OF PENICILLIN RETENTION IN THE BODY BY MEANS OF PARA-AMINOHIPPURIC ACID[1]

THE very great rapidity with which penicillin is cleared from the blood stream and appears in urine is a major disadvantage in therapy, and suggests that it might be eliminated by renal tubular secretion in addition to glomerular filtration. If such were the case, it might be possible to suppress the secretion of penicillin by the simultaneous administration of p-aminohippuric acid (PAHA) which is known to be secreted by the tubular epithelium[2] and which we have found to be remarkably non-toxic. Rammelkamp and Bradley have reported that the excretion of penicillin in urine was depressed by the injection of diodrast.[3]

The purpose of our investigations was to determine whether a mutual competition between penicillin and p-aminohippuric acid existed and, if so, to evaluate the significance of that relationship. The penicillin content of urine and plasma was determined by a modification of the method of Rammelkamp[4] and the total amounts recovered were checked by the Florey cup plate method. The PAHA content of urine and plasma was determined by making use of the principle set forth in the method of Bratton and Marshall for sulfonamides.[5] All urine and blood samples were collected aseptically and periodic renal clearance determinations of PAHA and penicillin were made during the course of the experiments. It was established that penicillin contained in urine was sufficiently stable to permit complete recovery in the presence and absence of PAHA at a pH range of 4.5 to 8.0 and that PAHA did not influence the assay of penicillin.

Two-hour experiments using normal, unanesthetized trained dogs were designed in which 10,000 Oxford units of penicillin were injected intravenously as a single dose. In the control tests no PAHA was infused, but in other experiments intravenous PAHA infusion was started shortly before and carried out continuously during the experiments. These experiments demonstrated that PAHA markedly prolonged the maintenance of an elevated plasma concentration of penicillin, being 0.2 unit at 2 hours compared to only a trace of penicillin in the plasma of the control animals at 1.5 hours. The recoveries of penicillin in the urine of one dog when no PAHA was administered were 61, 77 and 97.7 per cent. When PAHA was administered intravenously only 29.6 to 36.6 per cent. of the penicillin injected was recovered in 2 hours. In the case of another dog the control penicillin percentage recovery ranged from 64.9 to 102.4, whereas when PAHA was administered in addition to penicillin the recoveries of the latter were 30.2 to 52.6 per cent. When the former dog was given sodium bicarbonate by stomach tube to maintain the pH of urine at 7.8 to 8.0 the recoveries of penicillin were 107.8 per cent. for the control experiment and 36.1 per cent. when PAHA was infused. The normal renal clearance of penicillin at plasma concentrations of less than 1.3 units/cc approximated the minimal renal plasma flow. When the plasma level of PAHA was maintained at levels of above 25 mgm/100 cc the clearance of penicillin was depressed to and below the glomerular filtration rate for these dogs. This may be taken as evidence that penicillin and PAHA compete for the same tubular secretory mechanism.

Twelve-hour experiments, similar to those outlined above but during which the dogs were anesthetized, substantiated and extended the above findings. In the control experiments, with one exception, penicillin was no longer detectable in the plasma within 2.5

[1] From the Departments of Pharmacology and Bacteriology, the Medical-Research Division, Sharp and Dohme, Inc., Glenolden, Pa.

[2] (a) N. Finkelstein, L. M. Aliminosa and H. W. Smith, *Am. Jour. Physiol.*, 133: 276, 1941. (b) R. J. Bing, *Proc. Soc. Exp. Biol. Med.*, 53: 29, 1943.

[3] C. H. Rammelkamp and S. E. Bradley, *Proc. Soc. Exp. Biol. Med.*, 53: 30, 1943.

[4] C. H. Rammelkamp, *Proc. Soc. Exp. Biol. Med.*, 51: 95, 1942.

[5] A. C. Bratton and E. K. Marshall, *Jour. Biol. Chem.*, 128: 537, 1939.

hours following the single intravenous injection of 10,000 Oxford units. When PAHA was infused to maintain a plasma concentration of 30 to 50 mgm/100 cc, the end point at which the plasma concentration of penicillin was less than the lower limit of our test (0.02 unit/cc) was extended to 3.5 hours. Penicillin ceased to be excreted in the urine in determinable quantities within 7.5 hours during the control experiments but was still being excreted in small amounts at the end of 9.5 to over 12 hours when PAHA was administered continuously over this period of time. In these experiments, also, PAHA decreased the renal clearance and the recovery of penicillin in the urine as in earlier experiments.

Forty-eight-hour experiments were performed during which both penicillin and PAHA were infused continuously. As a result of these tests we found that, if the plasma level of PAHA was maintained at 20 to 30 mgm/100 cc, we could maintain a plasma penicillin level of 0.08 to 0.1 unit/cc when the penicillin was infused at a rate of 15 units/minute. On the other hand, when PAHA was not administered the penicillin level in the plasma was 0.02 units/cc or less. Also, by raising or lowering the plasma concentration of PAHA we could concomitantly raise or lower the penicillin content in plasma even though the penicillin was infused at a constant rate during the whole of the experiments. As the plasma concentration of penicillin was caused to rise by the intravenous infusion of PAHA there was a progressive decrease in renal clearance and urinary excretion of the antibiotic substance.

There appeared to be no pathological findings attributable to the PAHA-penicillin therapy in any of the 48-hour experiments. Toxicological studies have shown PAHA to be remarkably non-toxic to mice, rabbits and dogs; so much so that the physical characteristics of the high concentrations (20 to 40 per cent.) of the sodium p-aminohippurate solutions found necessary for intravenous toxicity studies were probably the principal causes of deaths which occurred at an LD50 dose of 5,300 mgm/kg administered intravenously to mice.

The detailed accounts of these experiments will be published elsewhere. These results indicate that with the aid of PAHA one may attain and maintain materially higher concentrations of penicillin in plasma than is practicable without the use of excessive amounts of penicillin. The ease and economy with which this can be accomplished experimentally suggest that the combined intravenous administration of penicillin and PAHA may offer sufficient therapeutic advantages to make its clinical trial indicated.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

A METHOD OF OBTAINING RENAL VENOUS BLOOD IN UNANESTHETIZED PERSONS WITH OBSERVATIONS ON THE EXTRACTION OF OXYGEN AND SODIUM PARA-AMINO HIPPURATE[1]

THE present concepts of human renal physiology are based upon somewhat unstable tenets because of the difficulty in obtaining blood from persons immediately before and after its passage through the kidney. The widely used clearance techniques for determining renal blood flow are based upon the assumption that the test substance used is completely removed from the blood during one circulation through the kidney. Arterial blood may be obtained with relative ease from a peripheral vessel, because its composition is the same throughout the body, but obtaining venous blood as it leaves the kidney has presented the major problem. In animals explantation of the kidney has enabled investigators to obtain renal venous blood for extraction studies. To our knowledge human renal venous blood has only been obtained during operative procedures on the kidney.[2] The method described here has enabled us to obtain blood directly from the renal vein in the unanesthetized resting subject. A similar technique is being successfully utilized by Dr. S. E. Bradley at the Boston University School of Medicine.

The method is essentially a modification of the technique of right auricular catheterization, introduced by Forssmann in 1929,[3] and recently extensively utilized by Cournand and his colleagues.[4] A long (100 cm) No. 8 or 9 radioopaque ureteral catheter[5] with an angulated tip is passed into the venous system through the antecubital vein. A slow drip of physiologic saline

[1] From the Medical Service of the Grady Hospital and the Department of Medicine, Emory University School of Medicine, Atlanta, Georgia. The work described in this paper was done under a contract, recommended by the Committee on Medical Research, between the Office of Scientific Research and Development and the Emory University School of Medicine.

[2] S. Weiss, F. Parker, Jr., and G. P. Robb, *Ann. Int. Med.*, 6: 1599, 1933.

[3] W. Forssmann, *Klin. Wchschr.*, 8: 2085, 1929.

[4] A. Cournand, R. L. Riley, S. E. Bradley, E. S. Breed, R. P. Noble, H. D. Lauson, M. I. Gregersen and D. W. Richards, *Surgery*, 13: 964, 1943.

[5] Obtained from the United States Catheter and Instrument Co., Glens Falls, N. Y.

solution is maintained through the lumen of the catheter during the entire procedure. Under fluoroscopic control the catheter is manipulated up the

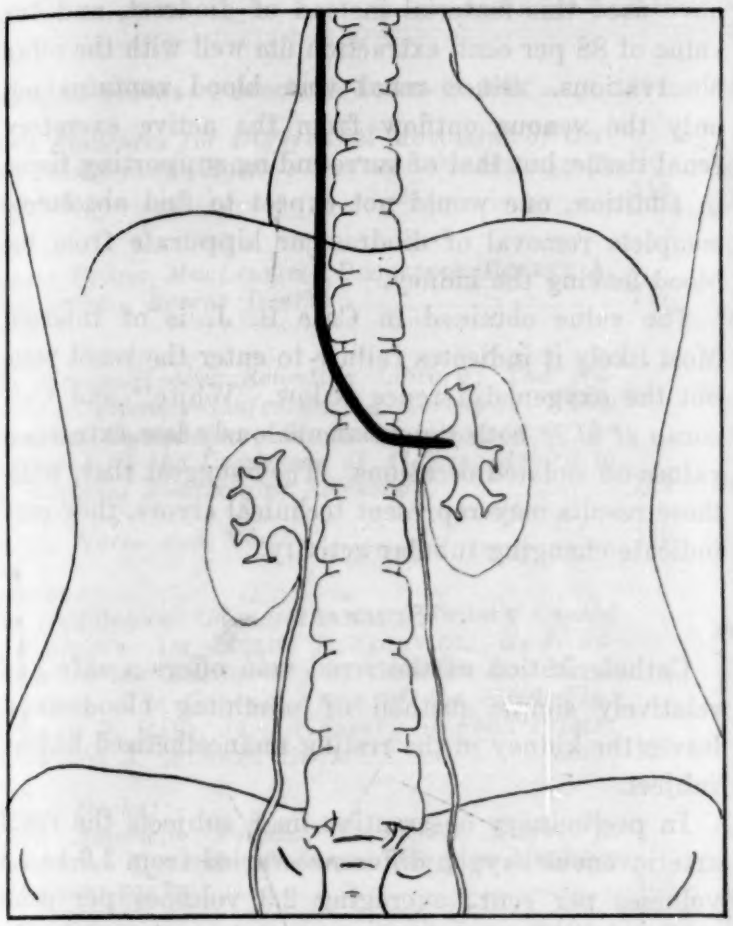

FIG. 1

venous system, through the right auricle and into the inferior vena cava. At the level of the renal pedicle the tip of the catheter is seen to pass laterally into the renal vein. When the catheter is in place the appearance is as shown in Fig. 1. Care must be taken that the catheter has not entered the hepatic vein.[6]

[6] J. V. Warren and E. S. Brannon, *Proc. Soc. Exp. Biol. and Med.*, 55, 144, 1944.

It is entirely possible that the vein entered may not be the renal vein, but one of the smaller veins in that locality. There are several methods of determining that one actually has entered the renal vein. By fluoroscopy the catheter is seen to be in position, as shown in Fig. 1, and to move as the kidney moves with respiration. The injection of diodrast or similar substance producing a pyelogram may be used to study the positions of the catheter. Samples of blood withdrawn from the renal vein have a relatively high oxygen content in contrast to venous blood from other sources, indicative of the low arteriovenous oxygen difference in the renal circulation. The most definite proof is the comparison of arterial and venous blood after the injection of one of the substances almost completely extracted by the kidney. We have used sodium para-amino hippurate[7] in low plasma concentration.

After the catheter is in the renal vein, and with the subject relaxed, simultaneous samples of blood may be withdrawn from the catheter and the femoral artery. In the cases reported here, after obtaining a sample for blank analysis, sodium para-amino hippurate was injected and 8 to 10 minutes later additional samples withdrawn for clearance studies. The catheter was then quickly withdrawn to the right auricle and a sample of mixed venous blood and expired air obtained simultaneously. This enables one to calculate the cardiac output utilizing the Fick principle. The concentration of sodium para-amino hippurate was determined by a modification[8] of the method of Bratton and Marshall[9] for sulfonamides. Subjects not receiving sulfonamides were used and it was found that much care was necessary in the use of procaine be-

TABLE 1

SUMMARY OF OBSERVATIONS ON EXTRACTION OF OXYGEN AND SODIUM P-AMINO HIPPURATE

Subject	Arterial O_2 content	Mixed venous O_2 cont.	Renal venous O_2 cont.	Cardiac A-V difference	Renal A-V difference	Cardiac index	Sodium p-amino hippurate		
							Arterial	Venous	Extraction
	Volumes O_2 per cent.			Volumes O_2 per cent.		Liters per min. per sq. meter	Mgm per cent.		Per cent.
B. S. ...	19.2	15.7	17.1	3.5	2.1	3.8	1.36	0.13	90
J. Y. ...	7.9	...	5.3	...	2.6	...	...	...	...
L. M. ...	20.7	14.6	18.2	6.1	2.5	2.3	0.21	0.00	100
B. J. ...	19.6	14.9	17.2	4.7	2.4	2.4	2.04	2.04	0
J. M. ...	9.1	5.4	6.8	3.7	2.3	3.8	1.68	0.21	88
A. B. ...	...	...	...	...	...	...	0.67	0.00	100
F. K. ...	6.5	3.1	3.7	3.4	2.8	3.7	1.39	0.18	87
J. W. ...	13.8	9.7	...	4.1	...	3.1	.95	0.11	88
H. B. ...	12.5	8.3	10.2	4.2	2.3	3.1	2.49	0.36	85
				...	...	...	1.56	0.24	86
M. G. ...	15.2	9.7	13.3	5.5	1.9	2.4	0.96	0.10	90

[7] Obtained through the courtesy of Dr. John Henderson, Sharp and Dohme, Philadelphia, Pa.

[8] H. W. Smith, Personal communication.

[9] A. C. Bratton and E. K. Marshall, Jr., *Jour. Biol. Chem.*, 128: 537, 1939.

cause these substances interfere in the analysis for hippurate.

Table 1 contains the results of studies on ten persons by these methods. The observations reported here were all made on subjects apparently free from renal disease. Several had considerable anemia, as reflected in the oxygen content of the arterial blood. The renal arteriovenous oxygen difference in the 8 cases in which it was determined varied from 1.9 to 2.8 volumes per cent. In contrast the difference in oxygen content of the mixed venous and arterial blood varied from 3.4 to 6.1 volumes per cent. The cardiac index in these subjects ranged from 2.3 to 3.8. With arterial plasma hippurate levels below 2.49 mgm per cent., the extraction by the kidney varied from 85 to 100 per cent. except for one case, when it was zero.

In all the cases reported here, in which oxygen studies were carried out, a low renal arteriovenous oxygen difference has been found. This was first noted in animals by Claude Bernard[10] in 1858. More recently Van Slyke *et al.*,[11] and Mason, Blalock and Harrison[12] have obtained similar values in animals with explanted kidneys. Weiss, Parker and Robb[2] have reported studies on renal venous and arterial blood from patients under spinal anesthesia during a surgical procedure for suspension of the kidney. The renal arterio-venous oxygen difference in three such subjects was 1.29, 2.87 and 2.96 volumes per cent., while in a patient with malignant nephrosclerosis the arterial and venous oxygen contents were 11.17 and 11.32 volumes per cent., respectively. Our results are of the same order of magnitude, but have been obtained under more physiologic circumstances. Other chemical studies are now in progress.

The use of diodrast clearance by the kidney to determine renal plasma flow is based on the assumption that at low plasma levels diodrast is completely extracted during a single circulation through the kidney.[13] The only direct evidence available at present is that from studies on animals with explanted kidneys. In such animals White[14] obtained an average extraction of 74 per cent. of the arterial content. Later Corcoran, Smith and Page,[15] using slightly lower plasma levels of diodrast, reported a value of 87 per cent. Recently it has been demonstrated that the clearance of sodium para-amino hippurate is identical with that of diodrast under normal conditions.[16] This substance has the advantage of a simple and accurate method of determination, plus the theoretical advantage of less diffusion into the red blood cells. We have used this material instead of diodrast, and our value of 88 per cent. extraction fits well with the other observations. Since renal vein blood contains not only the venous outflow from the active excretory renal tissue, but that of surrounding supporting tissue in addition, one would not expect to find absolutely complete removal of diodrast or hippurate from the blood leaving the kidney.

The value obtained in Case B. J. is of interest. Most likely it indicates failure to enter the renal vein, but the oxygen difference is low. White[14] and Corcoran *et al.*,[15] both report anomalously low extraction values on isolated occasions. They suggest that, while these results may represent technical errors, they may indicate changing tubular activity.

Summary

Catheterization of the renal vein offers a safe and relatively simple method of obtaining blood as it leaves the kidney in the resting unanesthetized human subject.

In preliminary observation on 8 subjects the renal arteriovenous oxygen difference varied from 1.9 to 2.8 volumes per cent., averaging 2.3 volumes per cent. Sodium para-amino hippurate, at low plasma levels, was 88 per cent. extracted during a single circulation through the kidney.

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